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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Natural and anthropogenic atmospheric mercury in the European Arctic: a speciation study" *by* A. O. Steen et al.

Anonymous Referee #3

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The paper by Steen et al. presents observations of GEM, RGM, and PHg from Ny-Alesund over a period of more than one year. The data should be published as it is important and will be useful to researchers. However, there are many conclusions, statements, etc. in the paper that are not well founded or at best confusing. For this reason, I believe the paper needs to be revised with an eye towards simplification and a focus on the observations. More detailed comments are listed below.

1) First sentence of the abstract "It is agreed. . .." Should be deleted if something is well founded in the literature it shouldn't be in the abstract.

2) Line 10 of abstract and later in the text. The authors state that a new seasonal

pattern of GEM, etc. I honestly don't know what this seasonal pattern is – I think they mean that you RGM throughout the summer and that PHg is only in spring. I am not sure this is a "new" seasonal pattern. There are observations of RGM outside of Spring ODE/AMDEs. For example see the work of Brooks at Summit, Greenland or work by Jaffe group, etc.. So I highly encourage the authors to state specifically what the important observations instead of couching it in these terms which are vague at best.

3) The authors state that BrO oxides GEM this is not the case. Br atoms are much more likely to the be the oxidant of GEM than BrO. This is misleading and the authors should review the likely oxidants of GEM and revise the paper accordingly. This is incorrectly stated in the introduction and abstract and should be changed.

4) The conclusions about the origin i.e. local vs. non-local AMDEs are really not supported by any data in this work. Or at least I don't understand the arguments. I really don't think that it is the origin or oxidant of GEM in this study is well understood. So conclusions along these lines are really speculative and should be minimized or at least softened.

5) The authors state in the last paragraph of the introduction that the EC model supplements the results of the study. I would say that it is used to analyze or understand the results of the study. The way it is phrased makes it seem that emissions from the model are providing the date in this work.

6) What are composite RGM and PHg measurements? A reference to an owner's manual is not appropriate. Please define the timing of these measurements more clearly

7) Bro columns from SCHIAMACHY were used to investigate BrO. However, the work by Salawitch et al., 2010 in a recent GRL demonstrate that much of the BrO column labeled as tropospheric is due to stratospheric variation. Has this been taken into account?

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8) The experimental details are sketchy – e.g. is stated that the soda lime is replaced every week again consistent with the Tekran manual. Either provide more details or reference a work that does. Also stating that a constant i.e. 8.33 is used to scale the data has no physical significance. This needs to be rewritten for clarityh.

9) The argument on page 27262 that observed RGM is somewhat too low because it is a factor of 3 lower than the EC model is not a statement that I would make. There are many reasons for such a discrepancy including model resolution, uncertainty in oxidation pathways.

10) Page 27262 line 12 should be “shorter” atmospheric residence time.

11) The reference to the “hump” in GEN in spring in Figure 2 is very hard to support by looking at the figure. So the ensuing discussion of GEM fluxes is hard to justify. Either Figure 2 data need to be shown with more resolution or a running seasonal average needs to be show to support his statement. I highly encourage breaking up Figure 2 into a couple of plots that allowed the data to be show with more resolution. This is the heart of the paper and it is very hard to make out. Other figures can be deleted to make up the space.

12) During polar night GEM concentrations are probably constant due to lack of chemistry as well as transport conditions (p. 27262 line 26)

13) I am not sure what Global Radiation means. I also think it is not surprising that RGM (which is short lived in the boundary layer) correlations with actinic flux? This is driven by photochemistry so this conclusion which is stated several times in the paper is not that surprising and could be stated once.

14) The statement that (p. 27263) that RGM was observed for the first time beyond spring time is simply not true.

15) Section 3.2 argues that spring RGM is due bromine – this may be true but satellite data is at best uncertain. A better argument would be to include ozone data (this should

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probably go in Figure 2) as ozone should also be reduced if bromine is present. Even this wouldn't be definitive but it would be a much better argument.

16) The whole discussion of MADE origin is confusing and in my mind at best speculative. I don't see how any conclusions can be made from the observations and the model. The oxidants and there distribution are too uncertain. The emissions map – Figure 5 – is not useful as far as I can tell. This discussion should be curtailed.

17) The correlation plot and analysis is not that useful. I would much rather the authors focus on some periods where the correlation coefficient is large and show this. As it is presented now in the figure it is very hard to follow.

18) I don't understand the third further research direction nor how it relates to the results in this paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 27255, 2010.